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CROUP—TRACHEOTOMY—RECOVERY.

[Communicated for the Boston Medical and Surgical Journal.]

On Friday, January 4th, I was called, at 10, A.M., to visit B—, aged 4½ years. On arrival, I found her suffering from “membranous croup.” The history of the case, as related by the parents, was as follows. “The Saturday night previous she appeared to have taken cold, and had a dry, hard cough. The next morning she appeared better, but commenced that night to be stuffed up and breathe with difficulty,” which symptoms continued to increase until I saw her. She had had no medical attendance. She was then breathing with great difficulty; dry, sonorous inspiration; hoarse, and unable to speak above a whisper; lips blue; eyes suffused; pulse rapid and feeble; considerable jactitation, and tendency to drowsiness. I gave an emetic of sulph. cupri, which soon operated, but with no relief. I stated to the parents that, in my opinion, the case was hopeless, as far as medical treatment was concerned, and suggested tracheotomy as affording the only, and that an extremely small chance for relief. At my request, they consented to have Dr. Kimball called in consultation. Her residence being several miles out of town, we were unable to get there until late in the afternoon, when we found the patient still more exhausted than in the morning, and concluded to operate at once. I opened the trachea with but little hæmorrhage; not having any tube expressly intended for the purpose, I substituted a piece of a large catheter, which answered very well. The patient did not seem at first to be any relieved, and we were not at all encouraged. We ordered tincture of veratrum viride, two drops every three hours; demulcent drinks; the atmosphere of the room to be kept moist; also gave directions to have the tube kept free with a feather, if it should become clogged.

Saturday, 5th, 10, A.M.—Found the patient slightly relieved; countenance better; pulse slower, and more full. During the

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morning, before I arrived, the tube had slipped out. I found the wound swollen, its edges everted and covered with a white exudation. The incision in the trachea was clear and open, air passing in readily. On taking everything into consideration, want of a proper instrument, &c., I concluded to let it alone so long as the wound in the trachea remained open and free. I gave another emetic of sulph. cupri, which in a few minutes expelled, by the mouth, several pieces of "false membrane," with about two ounces of very tenacious mucus. Ordered a cathartic of hyd. sub-mur., ipecac. and rhei; to have the wound occasionally sponged with tepid water; continue the veratria every four hours.

Sunday, 6th, 2, P.M.—Not as well; breathes with more difficulty than yesterday, but not with so much effort as before operation. Countenance pale and anxious. No discoloration of lips, or inclination to sleep. Wound in trachea open; two or three small shreds of "false membrane" protruding. Pulse more rapid and weak. Gave another emetic, which operated immediately, expelling some of the exudation with tenacious mucus. Appeared relieved after operation of the emetic. Ordered two grains carb. ammonia every hour; one teaspoonful of the following. R. Syr. ipecac.,  $\frac{3}{4}$  j.; syr. senegæ, syr. prun. Virg., aa  $\frac{3}{4}$  iv. M. To be repeated every two or three hours. Rubefacients to feet.

Monday, 7th.—Much better; slept quietly two hours during the night. Fever abated; skin moist; cough not so dry or urgent. Wound in trachea still open and free. Has some appetite; asked for, and may have, a cracker. Ordered olei ricini, half an ounce. The expectorant to be continued; also the sponging of the wound. Omit all other medicine. Directed great care to be used in order to keep the air in the room warm and moist.

Owing to sickness, I was unable to visit the patient again, but a messenger came daily to report her condition.

Tuesday, 8th.—Was quite bright and playful last evening. Slept well. Cough loose and expectorates freely; she is very hungry. Wound shows some disposition to close, but still admits air. Continue expectorant mixture p. r. n.. May have light nutritious diet.

Wednesday, 9th.—Doing well. Cough loose and subsiding. Spoke aloud to-day for the first time. The little patient continued rapidly to improve; the wound closed, and is now entirely well.

I cannot in justice close the report of this case without acknowledging the obligation I owe to Dr. Kimball, not only for his efficient aid in performing the operation, but for his prompt decision in regard to its expediency, without which the consent of the friends would with difficulty have been obtained.

HENRY B. C. GREENE, M.D.

*Saco, Me., Jan. 18th, 1861.*

## OBSERVATIONS UPON A MORBID CONDITION OF THE NECK OF THE BLADDER NOT DESCRIBED BY SURGICAL AUTHORITIES.

BY D. D. SLADE, M.D., BOSTON.

[Continued from p. 520.]

*Treatment.*—In our treatment, it must be our first care to ascertain the cause of the contraction, and according to the cause or causes, so will the treatment be either medical or surgical or both combined.

Whatever may be the therapeutical means employed, however, in every case it will be found necessary at the outset to accustom the canal to the presence of instruments, the introduction of which, under proper management, tends to overcome the morbid sensibility of the parts. It is therefore to surgical treatment, that we are obliged, in the great majority of cases, to have recourse, and of this we shall first speak.

As we remarked, when treating of the diagnosis of contraction, that it was by urethral exploration that we are to establish the presence of this affection, so in the treatment, it is highly important to understand the kind of instrument to be used, and the method of its employment.

Both for the exploration of the urethra in the establishment of our diagnosis, and at the commencement of treatment, we give preference, decidedly, to a medium-sized wax bougie. If we make use of this instrument, care should be taken to bend the end, for the space of half an inch or so, in order to avoid the sinus of the bulb, this portion of the canal in contraction being drawn downward and backward by the muscles of the perinæum; we may also advise the employment of the gum elastic bougie, with an olive-shaped button at the small extremity. This instrument is admirably suited to detect the smallest degree of contraction.

Whatever instrument may be selected, whether bougie or catheter, it is to be well lubricated with lard, cold cream, cerate, or some other equally tenacious substance, which is greatly to be preferred to the olive oil so commonly in use. Thus prepared, the instrument is to be introduced into, and carried along the urethra with the utmost care and gentleness. Should the patient complain of great pain in spite of these gentle manipulations, it is much better to desist at once, and remove the instrument, than it is to persevere; for this morbid sensibility of the urethra is to be overcome only by a very gradual treatment, and by great patience and perseverance on the part of both physician and patient.

The instrument should be introduced and passed every day or every other day, according to circumstances, and should be retained for a few moments only at the commencement. As the irritability of the canal is thus gradually overcome, the size of the instrument employed should be larger, and the use of it must be con-

tinued until the contraction is entirely overcome. An occasional passage of the bougie will be also requisite to keep the canal free.

We much prefer the gradual dilatation of contraction in the manner which we have described, to that which is produced by forced and sudden measures. In the former case, the method of treatment is slow, and in many cases tedious, but it is, at the same time, followed by more satisfactory results, than when violent means are adopted. We should, therefore, under no circumstances, advocate the forced method of dilatation.

All complications which may arise during the use of instruments, are, of course, to be met by appropriate treatment. Great care should be taken to avoid inflammatory action, which may be guarded against by a proper exercise of judgment as to the length of time and the frequency with which the instruments should be employed.

Cauterization is sometimes applicable in the treatment of contraction. In those cases where this affection has originated in, and has been kept up by gonorrhœal inflammation, this mode of treatment is often followed by good results, especially after other local and general measures have been tried. It is not applicable where the contraction depends upon any particular diathesis.

The mode in which cauterization is effected is worthy of a brief consideration. The nitrate of silver is the substance to which we give the preference, and instead of using *Lallemand's porte-caustique*, which is objectionable on many accounts, not the least important of which is its liability to become broken in the urethra, we employ an instrument of this description.

It is made like a common silver catheter; at the posterior surface of its vesical extremity is an opening, about three quarters of an inch in length, by two or three lines in width. This opening corresponds with the caustic which is placed in the cup attached to the *porte-caustique* in the interior of the canula or tube. Instead of melting the caustic into the cup, as is generally done when *Lallemand's* instrument is used, the cup is to be partially filled with cerate, tallow, or some similar substance, over which is to be sprinkled a thin layer of the powdered salt.

The instrument thus prepared is carefully introduced into the urethra, and carried down to the seat of the contraction. The inner rod or stylet, which was previously retracted, is now pushed on until the cup containing the caustic is opposite to the opening just described. By a rotary movement of the tube, the caustic is brought into contact with the parts which we wish to cauterize.

Cauterization, however, is a process to be avoided, if possible, in all cases where the urethra is implicated, and it is very rarely requisite in the treatment of contraction. In the cases dependent upon the cause just mentioned, and where the gradual dilatation has failed to effect a cure, we may frequently obtain very satisfactory results by a process much less formidable than cauterization.



This consists in the use of certain pomades, such as the red precipitate, double mercurial, iodide of potash, &c., passed into the urethra by means of an olive-shaped bougie. The space comprised between the olive-shaped button and the shaft of the bougie, for the space of half an inch or more, is to be filled up with the pomade to be used; the remaining portion being well lubricated with cerate. The instrument thus prepared, is to be carried rapidly down to the neck of the bladder, and allowed to remain for a few moments, until the ointment has melted, and has diffused itself over the neighboring surface. This process is to be repeated every second or third day, according to circumstances.

We have advocated the passage of the bougie or catheter, for the purpose of overcoming the morbid sensibility of the urethra; so frequently met with. In the great proportion of cases, if this is practised with care and judgment, it answers its purpose admirably. But it must be acknowledged that there are cases in which the bougie so much aggravates the irritable state of the canal, and thus increases the disposition to spasm, that it becomes impossible to persevere in its use. In such cases, this irritability must be considered as constitutional, and as dependent, as well as the contraction, upon some particular diathesis, which is, in the majority of cases, either the strumous or rheumatic. It follows, therefore, that to overcome the contraction, it is an essential condition that the constitutional vice be remedied, and we must consequently have recourse to medical treatment, of which we shall now speak.

In the constitutional treatment of contraction, either pursued alone, or in connection with surgical means, we must, of course, first of all pay strict attention to dietetic regulations, and to the correction of all abnormal states of the digestion, and secretions. If we have evidence of a congested condition of the pelvic viscera, we should employ suitable means for its removal.

General baths, douches of cold water upon the pubes, groins and perinæum, followed by thorough friction of the skin; care to keep the bowels free by the use of gentle laxatives or enemata; regular exercise—these are all decided adjuvants in our treatment. We recommend, especially, the use of sulphur baths, particularly in those cases which are dependent upon rheumatism. These should be taken as often as every other day—the patient remaining in the bath for an hour, and their use maintained until some decided effect has been produced.

The treatment of complications must not be overlooked, and all sources of irritability are to be allayed.

In the administration of drugs, our attention should be particularly directed to the peculiar diathesis upon which the contraction may depend. For example, in those cases where we have to deal with nervous affections, with the tubercular diathesis, with chlorosis, or with general debility, the use of tonics and alteratives is indicated. The preparations of iron, especially the muriated

tincture, and the iodide of potassium, in small doses, and maintained for a length of time, will give satisfactory results. Coupled with the general treatment which we may adopt, certain local means of cure may be employed. These consist of anodyne suppositories, opiate enemata, belladonna ointment, &c. These local measures are particularly valuable in allaying the morbid sensibility of the bladder and rectum.

The local application of electricity may be tried with good results. In a few cases, we have found that it has operated beneficially.

The internal use of belladonna in those cases where there is tendency to an irritable condition, and to spasmodic action of the urethra, is indicated. In the cases of children, who suffer from incontinence of urine, dependent upon contraction, the decided benefit resulting from the administration of belladonna cannot be denied.

Finally, in our treatment, we must not forget to advise the utmost attention to certain points which, though apparently insignificant, exert a marked influence upon therapeutics. Exposure to cold must be carefully guarded against. Flannel must be worn constantly, sexual intercourse is to be abstained from, all sources of excitement are to be avoided, and in certain cases the residence in a warm climate is to be taken up, if possible.

Although some cases of contraction are rebellious to any and to all treatment, which we may bring against it, still it will be found that the disease in itself will yield either to local treatment carried out with skill and judgment, or at least to local, combined with general curative means.

In a great many of the uncomplicated cases of this affection, the mental symptoms, of which we have spoken, soon pass away, as the treatment progresses. The mind of the patient becomes tranquillized, when he finds that the diagnosis is correct, and that there is really neither the calculus nor stricture, the existence of which he had such apparent reason to fear.

We have appended a few cases from our note-book, illustrative of the affection which we have endeavored to depict. Every-day's experience not only strengthens our opinion of the existence of such a disease as a permanent contraction of the muscular fibres of the surgical neck of the bladder, but that it is one which frequently presents itself to the notice of almost every practitioner.

We may hope that the observations of the preceding pages may have the effect to throw light upon a class of cases, which are often, very unsatisfactorily, set down by the practitioner as stricture of the urethra, or even as some more serious lesion of the urinary organs.

[To be continued.]

## THE VALUE AND THE FALLACY OF STATISTICS IN THE OBSERVATION OF DISEASE.

A BOYLSTON PRIZE ESSAY, BY DAVID W. CHEEVER, M.D.

(Concluded from page 517.)

SUCH are the quite positive conclusions that tubercles, in any organ of the body, after the age of fifteen years, involve their presence in the lungs: that chronic peritonitis, too, indicates pulmonary tubercles: that phthisis so often commences in the upper lobes, that we have been led to call those cases where the indications of its presence are found first at the base of the lungs, the *anomalous* development of tubercles. Again, how valuable is the knowledge that bronchitis and pneumonia oftener begin in the lower lobes, and that the former is mostly found in both lungs at once, but the latter only in one. No theory, and no speculation could ever have led to these results of numeration and averages. Such spontaneous creation of laws must have escaped our reasoning, because they do not agree with any of our preconceived opinions. Yet our ignorance of the conditions on which they depend is no bar to their utility. Although, then, the want of experiment, the presence of causal relations, from which we cannot disencumber facts, and the influence of various unknown powers modifying the phenomena of disease, must all moderate our expectations of the benefit of the numerical method, yet we have some useful results which cannot be done away with. So, too, in the typhoid affection, we find the characteristic lesion of Peyer's glands, the rose-colored eruption, the lassitude, the bleeding from the nose, the tympanites, as well as other pathognomonic symptoms, distinguishing it from typhus, marked out for us by the numerical method, not as invariable, but as present in the majority of cases.

So much accuracy has been conferred upon the student of medicine by physical signs, by chemistry, the microscope, and, to a certain extent, by the numerical method, that it has been asserted, that even Sydenham would have been but a smatterer beside the modern medical graduate. Yet, with all this, we venture to say that there have never been, and probably never will be in our profession, men of greater natural powers of observation, and of description, than Hippocrates, Sydenham, and Hunter, nor any who made better use of the light which their times afforded them.

Such a tendency has been awakened in later times to extend the numerical method to all branches of medical inquiry, that our periodical literature overflows with statistics, and every hospital annually tabulates the results of treatment. This is as it should be. And if we are often called to notice fallacies in the results of statistics, we can also record cases of the truly scientific employment of them. Such are to be found in the statistics of insanity, in the recent work of Messrs. Bucknill & Take on Psychological Medicine; and in a very carefully tabulated Consideration of

the Etiology of Continued Fever, by Charles Murchison, in the *Medico-Chirurgical Transactions*, Vol. XXIII. 1858. These leave nothing more to be asked from the careful employment of the numerical method, and carry its results as far as they are capable of going.

Louis himself mentions, as striking proof of the corrective tendency of the numerical method over the approximative one of simple experience, that, according to Corvisart, dilatation with thinning of the walls of the heart is common; but that, on counting in his book, only one case was found. So, too, Bertin and Bouillaud make the same assertion. Yet, in forty-five cases of heart disease observed by Louis, no instance of it was seen. Laennec, also, says that ulcerations of the trachea are common in phthisis, but uncommon in those who have not tubercles; yet, on numerical analysis, the very reverse is found to be the fact. But if this is sufficient to show the superiority of tabulated to remembered observation, it also indicates that morbid anatomy affords a much more profitable field for the numerical method, than the uncertainties of pathology can furnish. Both, indeed, must be as much superior, for its application, to therapeutics, as the science is superior to the art of medicine. For the additional uncertainties of treatment must still more prejudice its results.

Medicine has two provinces, to cure, and to prevent disease. The latter, though by far the most important, has ever been thought an inferior department, and been studied less than the former. Yet by how much is prevention nobler than cure! And it is in this department that most real progress has been, and can be made.

And here we may find the most profitable, as well as most certain application of the science of medical statistics. Etiology, as affected by vital and hygienic laws, has made more progress by the use of the numerical method, than in other ways.

The influence of a certain miasm, in producing intermittent fever, is so constant and invariable, that, like the contagion of smallpox, it requires no figures to prove it. Some few morbid agents are so constant as to need no calculation.

But it is the more doubtful ones that vital statistics have peculiarly enlightened. Such are the causes of typhoid and typhus fevers, of phthisis, of the spread and permanence of cholera and dysentery, &c. &c. The etiology of continued fever from bad drainage, cess-pools, and other poisonous influences, has been very well illustrated by the treatise of Dr. Murchison, before referred to. So, too, the returns of emigration, the reports of prisons, and the mortuary averages of over-crowded localities, have done much to indicate the way of prevention of typhus fever. Phthisis claims a much wider range of influences. But cholera and dysentery have, in later epidemics, been strikingly increased or diminished by certain local hygienic influences. In the localities most advanced in

hygiene, their rate of annual mortality has steadily decreased. Vital statistics are here invaluable. And we can well believe that "if the attention of society were once given to these points, the saving of life would be such as would not only modify our tables of mortality, but affect the fortunes of nations."

It is very true, also, that the general tendency of the use of statistics is to discourage *à priori* conclusions, and that they tend to exactness, both in the observer, and the facts observed.

Yet we can hardly afford to substitute, in all cases, mathematics for logic; arithmetic for induction, or calculation for reason, as M. Louis has been accused of doing. Even La Place styles theory "common sense applied to calculation;" and adds that reasoning, logic and induction are as useful in medicine as numbers. Such a method has been styled *eclecticism* by its author (M. Double); and, as an opponent to Louis, he sums up his argument as follows:

"Individuality is an invariable element in pathology; therefore every exclusive theory is absurd in pathology, and every absolute method repugnant to therapeutics. Numerical calculations, open to many sources of fallacy, are in no degree applicable to therapeutics."

A good deal of force is to be found, in opposition to the fallacy and the merely approximate nature of the numerical method, in the certainty derived from that mathematical formula, known as the *calculus of probabilities*.

Since the use of a very large number of observations, in every case, is impracticable, how shall we know what value to attach to statistical conclusions derived from a limited series of facts only?

By the calculus of probabilities; which must be received as demonstrated authority by those who do not choose to study it mathematically. This method proves to us that the probability of a given event's happening does not exactly coincide with the actual number of times it has been observed to happen, but varies between limits somewhat greater and somewhat less than the number observed; and that these limits, moreover, are wider in proportion as the observations are few, and approach nearer as the observations become more numerous. We subjoin the mathematical formula, which determines these results, taken from the work of M. Gavarret, by Dr. Bartlett.\*

\* If  $a$  represent the number of times that one of two events (call it A) has happened;  $b$ , the number of times that another event (B) has happened, and  $c$  represent the total number of observations collected, so that  $a$  plus  $b$  equal  $c$ : then the number which expresses the observed frequency of the event A, is not the true number, but merely an approximation to it, more or less close as the number of observations is greater or less. That number will, in any case, lie between

$$\frac{a}{c} + 2 \sqrt{\frac{2}{c} \frac{a \cdot b}{3}}$$

and

$$\frac{a}{c} - 2 \sqrt{\frac{2}{c} \frac{a \cdot b}{3}}$$

or at least, there are 212 chances to one in favor of its being comprised within those limits.

To take an example, and one from Louis himself. He has given, as the result of his treatment of 140 cases of the typhoid fever, 52 deaths, and 88 recoveries:  $52 + 88 = 140$ .

The mortality, therefore, might be supposed to be represented by  $\frac{52}{140} = 0.37143$ . Hence we should judge the mortality of typhoid, under the treatment of M. Louis, to be, approximatively, 37 deaths in 100 cases, or about .37 per cent.; a little more than one third.

Yet by using the calculations referred to, we shall find that the mortality may vary between the limits of

$$.37143 + .11550 = .48693, \text{ and}$$

$$.37143 - .11550 = .25593:$$

or approximatively, between .49 and .26 per cent.

In other words, that, by employing precisely the same treatment in a large number of cases of typhoid fever, we may lose from a quarter to one half of our patients; and not one third, as stated by M. Louis.

So, too, in comparing *any other* method of treatment with that of Louis, the aggregate sum of the conditions or circumstances remaining the same, it cannot be considered certain that the method is better or worse than his, unless the difference in the result exceeds these possible limits.

For to show the advantage of greater numbers of observations, we will take the following case :\*

Let us suppose that 500 cases of a given disease have been subjected to a given treatment, with the result of 100 deaths, and 400 recoveries : and another 500 cases of the same disease have been subjected to a different treatment, with the result of 130 deaths, and 370 recoveries. In the first class, the ratio of mortality is as 20,000 to 100,000 ; in the second class, this ratio is as 26,000 to 100,000 ; the difference between the two being 6,000 in 100,000. An application to these numbers of the law of probabilities shows, that the limit of possible variation is equal to 7,508 in 100,000.

We cannot reasonably conclude, therefore, that the first method of treatment is better than the second, because the difference in the result *falls below* the limit of possible variation by the calculus of probabilities ; a variation which may be the effect of chance. The number of cases is not sufficient for the answer sought. But, by extending our observation to twice the number of similar cases, in which the ratio of mortality remains in each class the same, we find the following results : The limit of possible variation, ascertained by the calculation of probabilities, when applied to a thousand cases, instead of five hundred, sinks from 7,508, in 100,000, to 5,306, in 100,000 ; which is *surpassed by* the observed difference in the ratio of mortality, this being as 6,000 in 100,000. Here then

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\* Bartlett's Philosophy of Medical Science. 1844.

we have a positive demonstration of the superiority of the first mode of treatment over the second; and this demonstration got solely by increasing the number of our observations.

These calculations, it should be remembered, have nothing to do with the nature of the facts observed, but solely with their number. As far as they can be carried into medical investigations, they are, therefore, invaluable. We have before attempted to show why they are not generally applicable. And it must be evident to all, that the great difficulty is in getting strictly comparable facts, and enough of them. For the mechanical exactness of the numerical method makes one suspicious of it, when applied to the notoriously imperfect science, and still more fallible art, of medicine. Comparable facts may be employed, in which the sum of possible causes remains the same; that is to say, which are comparable with regard to those influences under our control. If the degree of variableness of aggregates be limited by the calculation of probabilities, the individual facts composing the aggregates may be fixed enough to be comparable.

But the aggregate of possible causes must remain invariable; otherwise the whole calculation falls to the ground, or the law will be modified by the new element which has been introduced.\* Thus, in 1824 and 1825, the number of legitimate births in France amounted to 1,817,572. Of these, 939,641 were male, and 877,931 were female. During the same period, the number of illegitimate births was 140,566. Of these, 71,661 were male, and 68,905 were female. Among the legitimate births, the proportion of males is as 51,697 to 100,000; while, among the illegitimate births, the proportion is only as 50,980 to 100,000. Now the difference might have amounted to 391 in 100,000 births, without surpassing the limits of the law of probabilities; but it really amounts to 717 in 100,000 births. Some difference must exist, then, in the sum of the possible causes to explain this. And this difference can only be found in the fact of legitimacy, or illegitimacy. In the same way is to be ascertained the law of the average number of children born to each family. But here, also, changes in the physical, moral, or social condition of the people may alter the sum of possible causes.

Therefore, as a general deduction from the above, we have the following rule. Each series of relationships and phenomena must be fixed enough to be comparable; must consist of large numbers; the limits of variability must be determined by the calculation of probabilities; and the sum of possible causes must continue uniform. The law will be positive in proportion to the completeness of the above conditions.

Yet, with all these discouraging requisitions, the faculties of comparison and generalization still remain divine attributes, and

\* Elisha Bartlett, *op. cit.*



those which proudly distinguish the human intellect from the instinctive and ever-uniform acts of the brute creation. If the vastness of the observations to establish a law must bear a fair proportion to the vastness of the circumstances controlling, or at least concomitant, under which the phenomena occur, yet we should consider that, in proportion to the complexity of the phenomena, is augmented the number of relations in which they *may* be surveyed and observed. All practical expedients and empirical rules are not to be neglected because they have not been rigorously defined and limited by the numerical method. For all common rules of the medical art have been ascertained and established by a series of observations of such vast extent as to compensate, in a great degree, for the absence of the other conditions of mathematical induction. The benefits of opium in pain; of mercury in secondary syphilis; of quinine in intermittent fever, or of arsenic in the papular or squamous affections of the skin, have had the nearly unanimous testimony of all observers in their favor, for successive ages.

And the same thing is true of most of the generally admitted rules of practice. They rest upon the concurrent testimony of immense numbers of witnesses, and of an almost indefinite number of observations. Here, then, we have an instance of the value of simple experience. But it must be the experience of multitudes of observers, and of long series of years. Both here, and in the result of the calculation of probabilities, the uncertainty is rendered so unimportant as to be practically disregarded. The corrective influence of multiplied observation is, in these instances, analogous to that geometrical problem by which we can indefinitely approximate the sides of a polygon, inscribed within a circle, to the circumference of the circle, until it shall be impossible to distinguish the one from the other; and the polygon has, to all intents and purposes, become a circle. Yet here, after all, we fail of the certainty of demonstration, and the result is, at best, *approximate*.

So the most careful methods which we may apply to the solution of medical problems are not only often utterly fallacious, but when most perfect, like this weaker part of geometry, are still inexact.

The inherent contradictions of medical science have been enhanced by the most opposite theories in all ages.

All the theorists say to the practitioner at the bed-side, "Do not try, but think; reason, argue, deduce!" Empirical Hunter said, "Do not think, but try!" So the modern disciples of the numerical method would say to us, "Neither think, nor try; but *calculate!*"

Meanwhile the patient dies. The average mortality, not only of the whole race, but of many acute diseases, remains unchanged century after century. Truly, when we consider the fallacies of

medical science, its confusion, its contradictions, and its impracticable theories, as well as the weakness of the medical art, and the little which it really can do; and when we contrast, with these humiliating considerations, its high aims, and exalted calling among the other branches of human knowledge, we may well say, as has been said, with epigrammatic brevity, "*La médecine est la plus noble des professions, and le plus triste des métiers.*"

Yet the truly physiological and scientific practitioner, trained to the finest edge of acumen, and, above all, taught to observe everything, is the man for the times. For the study and analysis of phenomena, and their relationships, and not the discovery of any general law like gravitation, marks those who are the Newtons of medicine. So did Hippocrates, Sydenham, and Hunter, Laennec, Andral, and Louis. And so should we all. The science of medicine wants facts; comparable, numerous, well observed, carefully arranged, minutely classified, and acutely analyzed. But little reward awaits those who collect them. He who devotes himself to the science of medicine must expect little sympathy from the mere votary of the art.

His reward lies in posterity, and the test of his conclusions must be in the future. No other agent but the lapse of time, can rightly estimate the varied elements which constitute the science and the art of medicine. This alone can finally arbitrate between the claims of statistics, and of the other methods of observation. So says Bacon:

"RECTE VERITAS TEMPORIS FILIA DICITUR, NON AUCTORITATIS."

### Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY FRANCIS MINOT, M.D., SECRETARY.

Dec. 10th.—*Dislocation of the Hip: Manual Reduction.* Dr. FIFIELD reported the following case:—

William Condrick, aged 10 years, while coasting, on the evening of Dec. 9th, 1860, was attacked by two boys, who pulled him from his sled, and jumped upon him, whilst his right leg was widely separated from the left. I saw him a half hour after the accident, lying in bed upon his back. The limb was greatly inverted, the great toe resting on the top of the ball of the left great toe, the right knee strongly in apposition with the left, from which it could not be much abducted. Measurement from the anterior superior spinous process of the ilium to the tip of the external malleolus gave half an inch shortening. The head of the femur could be indistinctly felt in the region of the ischiatic notch. Supporting the patient in an erect posture, the injured limb was seen to be widely separated from the other, not inverted in any marked degree, the foot pointing straight downward, resting on the toes; but the heel did not quite reach the floor. Satisfied that I had a case of dislocation into the sciatic notch to deal

with, I caused two firm mattresses to be put on a stout bedstead, and the boy placed upon them. I then sent for the assistance of my friend, Dr. Gilbert, of East Weymouth, who arrived at 8 o'clock, two hours having elapsed since the accident. Although I had previously determined to attempt reduction by manipulation, I arranged the compound pullies in case of failure. Dr. Gilbert was requested to administer a sufficient dose of ether to produce complete relaxation. The patient having been pronounced ready, I stepped quickly to the bedside, and taking the knee and ankle in my hands, I flexed the thigh on the pelvis, so as almost to touch the body, bent it over towards the left ilium, rotated it outwards, gave a slight lift with the hand at the knee, when, with an audible shock, the head of the bone left the notch, and came upon the dorsum of the ilium. I then depressed the whole limb, and with another audible shock it entered the socket. Measurements were now taken, and found to agree on both sides. The knees and ankles were secured together, and the patient left in bed for a week, the action of the bowels being checked for that length of time by opiates. He was then allowed to sit in a chair, with a stool of proper height beneath his feet. At the end of a fortnight he walked with assistance. He is now rather lame, but walks tolerably well without cane or crutch.

Three points in this case are worthy of notice:—1st. The position of the limb when the boy was held erect. The plate of dislocation into the sciatic notch, given in Sir A. Cooper's work on Dislocation and Fractures, represents a man standing erect, leaning on a support with his right hand. The right knee is in *opposition* with the left, the great toe rests on the ball of the opposite. In the present case the injured limb was widely *separated* from the other, when the boy was held erect. The great toe, instead of pointing across the ball of the opposite one, was directed downwards, and the foot rested on the toes. The only resemblance left to the plate, was, that the heel did not touch the floor. Why was this the only point of likeness left? Because it was the only thing left unaltered in its relation to the body by the altered position. Shortening (if I may be allowed the expression) is shortening, *must be* shortening, in any position of the body. Why was every other point of resemblance to the plate gone? Because the artist, having portrayed the position assumed by the limb when the subject was supine, imagined it must be the same when erect, which it is not. Whoever will attempt to support himself on one leg, will find that he leans toward that side, and that the opposite leg is thrust outward; that is, the pelvis has moved on the immoveable head of the firmly planted limb. Thus, in this case, when the patient stood erect, it was not that the relation of the injured limb to the pelvis was altered, but that the relation of the pelvis to the sound limb was changed.

Secondly. Another point of interest is the small amount of force required to effect reduction. I do not think more strength was exerted than would have sufficed to lift six or eight pounds from the ground.

Thirdly. I have said that the final reduction from the dorsum of the ilium to the socket, was effected by depressing the limb. From subsequent experiments with the skeleton, I think it would have been more philosophical to have everted the limb still more strongly, as in that position the head of the femur more nearly approaches the socket

than in any other. Effected in the former way, a slight pull would seem necessary.

Dr. H. J. BIGELOW said that it was convenient to remember, during the various complicated movements of the leg, in attempts at reduction, that the head of the femur always points in the same direction as the internal condyle.

In regard to Dr. FIFIELD's interesting case of luxation of the hip, Dr. B. directed attention to the importance, after flexing the thigh, so as to carry the head after the femur behind the socket, of either twitching or slowly lifting the femur strongly upward, during abduction, so as to jerk or lift it over the edge of the acetabulum into its place. This lifting effort must be proportioned to the weight of the limb, and is more important than abduction. The thigh being raised to a perpendicular, as the patient lies, and the knee bent at right angles, the leg becomes a powerful lever by which to rotate the thigh so as to point the head of the bone either above, below, or directly at, the acetabulum; its actual direction being easily recognized by observing that of the internal condyle to which it corresponds.

*Other Dislocations.*—Dr. H. J. BIGELOW said that reduction is sometimes impeded by the state of the capsule, which may be slit on one side, so as to allow displacement of the articulation, while the remaining fibres hinder reduction. This is especially illustrated on the smaller joints, for example, the phalanges. In such cases it is not muscular action alone that prevents extension; and the surgeon is surprised to find, after complete etherization, that the joint is still unyielding and irreducible, except by strong and protracted effort and manipulation. Dr. B. had repeatedly seen this occurrence in the shoulder, an efficient mode of reducing which luxation he would mention as new. The patient sitting on the floor, and the arm being raised above the head, the surgeon, standing in a chair, draws the arm vertically upward, at the same time depressing the clavicle and scapula with his uncovered foot upon the patient's neck;—or, still better, the patient lies, while the surgeon sits upon the floor, both occupying the relative positions already described. If the luxation does not yield, let the patient, still lying on his back, and the operator remaining stationary, be slid round upon the floor by an assistant, through a quarter circle, so that the extension, at first upward in the axis of the body, can be maintained until the arm is at right angles with it. This is best done with a rug on a smooth floor. If, now, the right clavicle and scapula of the patient have been depressed by the surgeon's left foot (or, *vice versa*, for the other side), the patient's arm extended at right angles with the body, lies at this moment between the knees of the operator; whose left foot on the neck and shoulder can be replaced by his right foot in the axilla, while the rotation of the patient on the floor is continued by the assistant, so as finally to bring the patient's arm to his side, the surgeon's foot being in the axilla, and the extension downwards. In this way, extension is maintained most advantageously from first to last, the arm slowly traversing an arc of  $180^{\circ}$ . Dr. B. had succeeded in this way, in a difficult case.

DEC. 10th.—*Fracture of the Cervix Femoris.* Dr. JACKSON showed the specimen, which Dr. STORER had recently obtained from Dr. H. E. CLAP, of Wrentham, and read the following history of the case which was subsequently received:—"In August, 1841, I was called to Mrs. K., and learned from her that in January, 1840, while standing upon a

low chair, it tipped, and she came to the floor. She was at that time 80 years of age, and she never walked after the fall, except by placing a low chair before her, and pushing it along; being, for some reason, unable to make use of crutches at all. The limb was shortened three or four inches. Little or no pain was suffered for two or three years, but for the two years previous to her death there was, at times, extreme pain in the whole limb, apparently of a neuralgic character, and readily yielding to three or four grains of conium. Before I saw her, the case was treated as a dislocation upwards and backwards, but I felt confident that there was a fracture. Until her last illness she enjoyed excellent health, and died, Aug. 4th, 1845, at the age of 85 years; there being loss of speech for eight days before her death."

The neck of the bone has been entirely absorbed, as usual when the patient has lived so long after the accident as in the present case; the new articular surfaces being uneven, with some eburnation, especially upon the shaft. Upon the original articulating surface of the head are several deposits of new bone, altogether considerable in amount, and such as are often seen in chronic arthritis.

DEC. 10th. *Large Biliary Calculus, discharged from the Bowels, after Symptoms of Obstruction.* Dr. JACKSON exhibited the specimen, which was also sent by Dr. CLAP, of Wrentham, with the following account of the case:—"I was called to Mrs. F., in Sept., 1846, and found her vomiting, with severe colicky pains in the bowels, small pulse, and cold extremities. Heat and stimulants were applied externally, and as there was great constipation, I gave her calomel with opiates, but without any effect. Counter-irritation over the epigastrium was also used. Combinations of cathartics with opiates were given, with large injections, but without any relief, the vomiting and pain continuing. She had been vomiting twenty hours, when I gave her large doses of Rochelle salt, with a teaspoonful of the tincture of hyoscyamus after each. In less than two hours a change for the better took place, and in three hours the vomiting ceased. I then ordered a large injection of Castile soap and water, and in a short time there was a copious discharge, soon followed by another in which was the calculus. The patient was then 66 years of age, and she is still living and enjoying excellent health, never having had an attack like the one above described, before nor since. She is not of a bilious temperament."

The calculus is extensively broken away upon one side, but seems originally to have been of a cylindrical form, about three-fourths of an inch in diameter, and varying in length from five-eighths of an inch to one inch; and each extremity is worn down over its whole surface into a very marked facet, as if by friction against other large calculi. The structure is quite compact, the color generally dark-brownish, and the surface finely granulated.

DEC. 31st.—*Disease of the Stomach; Chronic Inflammation or Malignant Disease?* Dr. JACKSON showed the specimen, in a perfectly fresh state, which had been sent to him by Dr. George C. Lincoln, of South Malden, with a history of the case. The organ, when removed, was "small, whitish, and cartilaginous to the touch, and on attempting to separate it, a strong band of thickened omentum was found attaching it to the transverse colon." To the extent of from one to two inches from the pylorus, the parietes were quite healthy, and also, to a much smaller extent, near the cardiac orifice. The muscular coat was quite firm, red, and moderately thickened. The mucous and sub-

mucous cellular coats\*were so perfectly blended, that no trace of their limits could be seen; and they were everywhere more or less thickened, and generally much so;—one of the most striking peculiarities of this form of disease of the stomach, which Dr. J. was inclined to regard as not exceedingly rare, being the existence of numerous, and quite marked elevations upon the inner surface of the organ, of a more or less elongated, oval form, and due to the greater thickening in some parts than others of the tissues now referred to; and these last tissues were tough, rather than dense, as in a common "scirrhus stomach." Upon the anterior face, and towards the cardia, there was an ulcer, about an inch in diameter, rather ragged, and penetrating nearly or quite to the muscular coat; being recent in appearance, and quite different from the ulcers that are so common in "scirrhus" of the organ. Otherwise, the mucous surface was in no way remarkable, excepting a considerable quantity of transparent, and very viscid mucus. There was no trace of any further ulceration, of lymph, or of the soft fungous growths so often seen in "scirrhus."

Dr. J. said that he had never been able to satisfy himself in regard to cases like the above, but he was rather inclined to consider them as a modification of the form of disease that is so particularly apt to affect the pyloric portion of the stomach, and which he regarded as malignant, though not cancerous microscopically.

Dr. L. found considerable œdema of the extremities, and some slight morbid appearances in other organs than the stomach, but having no bearing on the case.

*History.*—The patient was a woman, 67 years old, and generally healthy. Fifteen years ago, she is said to have been dyspeptic for some months. About a year ago her sickness began, with some pain and other trouble at the epigastrium, with anorexia and lassitude. She was then rather fleshy, of a florid complexion, and quite healthy in appearance. From the above symptoms she recovered, under treatment, and considered herself as well as usual. Last April she began to complain of uneasiness, and of a deep-seated pain just below the left nipple, and extending to the sternum, felt particularly after eating, and obliging her sometimes very soon to stop. The pain was also increased by acids. From this time she steadily lost flesh. Four months before death, the pain and distress decidedly increased, with entire loss of appetite, and some fever in the afternoon. The mucous membrane of the mouth and fauces was red, with considerable expectoration of glairy mucus; and for about ten days there was considerable diarrhœa, which was finally checked by opiate enemata, the dejections being, on one or two occasions, slightly dysenteric; there being also considerable pain, though no tenderness, in the abdomen. At this time there was no nausea, except immediately after taking more than a certain amount of food, which last was never thrown up. During the last six weeks there were more frequent attacks of retching, and considerable quantities of tough mucus were thrown up; on two or three occasions this last was tinged with blood, and then only the odor was quite fetid. During the last four weeks, the appetite returned, but only the smallest quantity of food could be taken at a time. The pain was controlled by morphia, no more than a grain in twenty-four hours being ever required. The bowels were readily moved by enemata, and the constipation was never severe. There was some return of diarrhœa during the last week. She sank gradu-

ally, and "the countenance never assumed the decided cachectic hue."

Dr. L. remarked upon the large amount of disease found in the stomach, upon the absence of any distinct tumor, and upon the pain, which was never sharp, but rather "a distressed ache." "The diagnosis was obscured by the general condition of the patient, and yet the symptoms indicated clearly the stomach as the seat of disease; and the conclusion was that it must be scirrhus of the cardiac portion only."

Dr. J. remarked that though, as above stated, his impressions would be in favor of malignant disease anatomically, the symptoms seem rather to point to chronic inflammation, in explanation of the nature of the disease.

## THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON: THURSDAY, JANUARY 31, 1861.

THE following communication, embodying certain resolutions recently adopted by the New York Academy of Medicine, with reference to the publication of its proceedings in the daily papers, are not without interest, as showing that a decided step has been taken in advance by that influential society. The voice of such a body, expressed in so emphatic and decided a manner, will have its effect in checking a practice which is not only in direct violation of the rules adopted by the American Medical Association, but any encouragement of which, however slight, is unworthy, to say the least, of those who have been admitted to the honors of a liberal and learned profession.

MESSRS. EDITORS.—At a stated meeting of the New York Academy of Medicine, held on the 16th inst., after the inaugural address by James Anderson, M.D., and the reading of a paper by John Watson, M.D., the Academy went into executive session. The following preamble and resolutions, after a free discussion, were adopted by a very large majority:—

*Whereas*, Section 3d, Article 1st, of the Code of Ethics of the American Medical Association, adopted by this Academy, declares it to be derogatory to the dignity of the profession to publish cases and operations in the daily prints, or to suffer such publications to be made, and that such proceedings are the ordinary practice of empirics, and highly reprehensible in a regular physician; therefore,

*Resolved*, That the practice, heretofore in vogue in this Academy, of permitting reporters of secular papers to attend its meetings, take notes of its proceedings, and publish them, is a violation of such code, and is hereby prohibited in future.

*Resolved*, That it is equally in violation of said Code, for Fellows of this Academy to publish, or cause to be published, any transaction thereof, in any other than strictly medical journals.

*Resolved*, That nothing in the aforesaid resolutions shall be so construed, as to preclude this Academy, at its discretion, from publishing in the newspapers any of its discussions, or of its acts, relating to hygiene, or to public health.

When the vote was taken, there was but one dissenting voice, and this was from a Fellow who confessed that he had been in the habit of



furnishing reports of the proceedings for one of the daily papers. He contended, most strenuously, that these reports had been of immense advantage to the Academy, as thereby it had been made known throughout the length and breadth of the land. He confessed also, in reply to a question, that they had been of *advantage to himself*, as he had been in the habit of receiving *five dollars a column* for matter so furnished.

You, Messrs. Editors, will rejoice with us at this result. The effort to remedy the evil originated long since with certain Fellows of the Academy. They desire now to acknowledge the valuable aid rendered by your Journal, as well as the New Jersey Medical and Surgical Reporter, and last, though not least, our own American Medical Times. These journals have nobly and fearlessly discharged their duty. They deserve and will receive the thanks of all the friends of legitimate medicine.

CONNECTICUT.

A SENSIBLE SHOE.—We have often heard old people, who have outlived their vanity, talk about “sensible shoes,” by which phrase they intended to convey the idea of long, wide, leather receptacles, too large for the feet. This view being too repulsive to the minds of those who had more æsthetic ideas, has not been generally adopted. Unfortunately the latter have forgotten the danger of forming a shoe upon the principles which guide them in the construction of a bonnet. To vary the shape, as is constantly done, without regard to the conformation of the foot, is sure to be followed by deformity and all its attendant sufferings.

Dr. Plumer, of Portland, has designed a last upon what, the most skeptical will allow, is, at least, a correct principle. He has taken the foot itself as a model, and given it support where the latter is most needed, and avoided pressure which could only be injurious. The principal improvements are in the shape of the sole and the position of the heel, and we feel persuaded that the adoption of them would add much to the comfort of those who

“Sow in suffering what they reap in corns.”

NEW MEDICAL JOURNAL.—The establishment of a new medical periodical is by no means a rare event, and one too often marked by little that is worthy of more than a passing notice. *The Baltimore Journal of Medicine*, the first number of which has just been received, we are led to regard, from the character of those interested in its support, as well as from the fact that it is, so far as we are informed, the only medical journal in the city of Baltimore, as an important addition to this department of our medical literature. The Editor is Prof. Edward Warren, of the University of Maryland, and the editorship could not be in more competent hands. The articles in the present number are well written, and carefully selected, and give an earnest of what the friends of the Journal may expect in the future. With Dr. Warren at its head, and some of the first men in the country pledged to its support, we have little doubt of its entire success.

WE regret that we are obliged to omit, this week, the continuation of Dr. Ware's Lectures on General Therapeutics. We hope hereafter to present to our readers a portion of this valuable contribution in each week's issue.

M. THIERCELIN presented a paper to the French Academy on the 12th of November last, giving his experience with Woorara in the treatment of epilepsy. He reports two cases of several years' duration which were greatly benefited by the use of this agent.—*Baltimore Journal of Medicine*.

PHOSPHORNECROSIS has become prevalent among the makers of lucifer matches in France. The Academy, at the solicitation of the Government, recommends, as a means of prevention, that matches be made of pure amorphous phosphorus.—*Ibid*.

THE SMALLPOX exists to a fearful extent on board the corvette Cumberland, of the home fleet, although she has been only a short time in service.—The eulogy of the late M. Achille Richard, of Paris, the eminent botanist and professor of the faculty, was pronounced by M. Dubois, Perpetual Secretary to the Academy, on the 15th of December.

#### VITAL STATISTICS OF BOSTON.

FOR THE WEEK ENDING SATURDAY, JANUARY 26th, 1861.

##### DEATHS.

	Males.	Females	Total.
Deaths during the week, . . . . .	32	33	65
Average Mortality of the corresponding weeks of the ten years, 1850-1860, . . . . .	40.9	33.8	74.7
Average corrected to increased population, . . . . .	..	..	83.3
Deaths of persons above 80, . . . . .	..	..	..

##### Mortality from Prevailing Diseases.

Phthisis.	Croup.	Scar. Fev.	Pneumonia.	Measles.	Smallpox.	Dysentery.	Typhoid Fever.
12	0	2	2	0	1	0	0

##### METEOROLOGY.

From Observations taken at the Observatory of Harvard College.

Mean height of Barometer, . . . . .	30.166	Highest point of Thermometer, . . . . .	89°
Highest point of Barometer, . . . . .	30.700	Lowest point of Thermometer, . . . . .	10°
Lowest point of Barometer, . . . . .	29.640	General direction of Wind, . . . . .	N. & N.W.
Mean Temperature, . . . . .	24°.8	Am't of Rain (in inches) and melted snow . . . . .	3.344

From Observations taken by Dr. Ignatius Langer, at Davenport, Scott Co., Iowa. Latitude, 41.31 North. Longitude, 13.41 West. Height above the Sea, 585.

		BAROMETER.				THERMOMETER.				SNOW.		Mean Amount of Cloud. 0 to 10.
		7 A.M.	2 P.M.	9 P.M.	Mean	Lowest Point.	7 A.M.	2 P.M.	9 P.M.	Height, 15 in.	Mean Sifted, 0.85.	
Monday, Jan. 14,		29.34	29.37	29.37			33	36	35			
Tuesday, " 15,		29.25	29.01	28.72			34	34	33			
Wednesday, " 16,		28.79	29.15	29.40			30	32	24			
Thursday, " 17,		29.52	29.25	29.42			21	27	26			
Friday, " 18,		28.90	29.01	29.18	29.38	28.70.	25	28	25			
Saturday, " 19,		29.35	29.46	29.52			13	19	12			
Sunday, " 20,		29.54	29.61	29.65			0	12	3			

COMMUNICATIONS RECEIVED.—Case of Croup—Tracheotomy and Recovery.—On Diphtheria.

BOOKS RECEIVED.—Ergot: Its Natural History and Uses as a Therapeutic Agent. By E. N. Chapman, M.D., Philadelphia.—On Diphtheria. By Edward Headlam Greenhow, M.D., F.R.C.S., &c. (From Baillière Bros., 440 Broadway, New York.)—Braithwaite's Retrospect.

DIED.—At Madison, Wis., August 28th, 1860, of consumption, Dr. Joel Rice, formerly of Bridport, Vt., aged 65 years.

DEATHS IN BOSTON for the weeking Saturday noon, January 26th, 65. Males, 32—Females, 33.—Accident, 2—apoplexy, 2—inflammation of the bowels, 2—disease of the brain, 1—Inflammation of the brain, 1—bronchitis, 1—consumption, 12—debility, 1—diphtheria, 2—dropsy, 3—dropsy of the brain, 5—epilepsy, 1—erysipelas, 1—scarlet fever, 2—haematomesis, 1—disease of the heart, 5—disease of the hip, 1—disease of the liver, 2—Inflammation of the lungs, 2—marasmus, 1—old age, 2—paralysis, 1—spina bifida, 1—smallpox, 1—suicide, 1—tuberculosis, 1—unknown, 9—whooping cough, 1.  
Under 5 years of age, 24—between 5 and 20 years, 7—between 20 and 40 years, 13—between 40 and 60 years, 12—above 60 years, 9. Born in the United States, 47—Ireland, 14—other places, 4.

